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cussion of the moorhen's first dive and Yerkes's and Bloomfield's experimental observations on the behavior of kittens in killing mice. In order to disprove the existence of delayed instincts, it is not sufficient to indicate that elements of the response have been exercised before. It is necessary to account for the somewhat sudden grouping of the elements into a significantly new response under conditions where the influence of habit formation is experimentally controlled and negligible.

The opponents of instinct will also have a very considerable difficulty in handling such data, meager though it is, as that presented by Yerkes on the inheritance of savageness and wildness in rats and by Whitman on the hybridization of pigeon behavior.

There are other interesting—but I think seriously mistaken—points both in Mr. Kuo's paper and in other recent papers couched in a similar vein. My purpose, however, is merely to remove the misapprehension which has come to my notice with reference to an earlier proposition, *viz.*, that associations formed prior to the appearance of an instinct may modify the instinct when it appears.

WALTER S. HUNTER.

THE UNIVERSITY OF KANSAS.

BOOK REVIEWS

Mysticism, Freudianism and Scientific Psychology. KNIGHT DUNLAP. St. Louis: C. V. Mosby Company, 1920. Pp. 173.

There are two main points of interest in this stimulating essay: the pigeonholing of the Freudians with the mystics; and the reflex arc concept applied to perception and to the association of ideas.

Mysticism is illustrated by quotations from Plotinus and Dionysius, as well as from Maeterlinck and other modern mystics. It pretends to reach a "third kind of knowledge," additional to that gained through the senses and through inference.

This third and highest type of knowledge is to be had only in rare moments of absorption in an adored object, and is ineffable and incommunicable. The author concludes that this "knowledge" amounts to emotion, pure and simple, with the sex element strong in the emotional complex. But such emotional experience has no claims to scientific recognition as a source of knowledge.

"Pseudo-mysticism," exemplified by belief in spirit communication and telepathy, differs from the genuine article in pretending to employ logical inference in reaching its conclusions, but its logic is constantly vitiated by the use of ambiguous middle terms, or "sliding terms," to use the author's expressive words. All mysticism is impatient of clearly defined terms, and of the whole painstaking procedure of science, and tries to solve the mysteries of life by uncritical short cuts.

Scientific procedure is strenuous and exacting. It requires constant touch with facts, parsimony in hypotheses, experimental testing of hypotheses, verification of results by independent observers, and careful definition of terms.

Freud is shelved with the mystics by the canny bookseller; Freud's popular appeal is to the love for the mystical. He and his followers shrink from the arduous road of science, and pretend to possess knowledge gained in some other way. His reasonings are vitiated by sliding terms. "Libido" is one such term; it means either the genuine sex motive, or something, oh! very much broader, just according to the momentary needs of the argument.

But the champion sliding term is *the Unconscious.* Sometimes the unconscious is simply a working hypothesis which proves to work well; and sometimes it is a fact from which there is no escape. Sometimes it is merely a "negative concept which can neither be described nor defined"; and sometimes "we are already familiar with a whole series of positive distinguishing features which differentiate the unconscious psychic material from the rest, the conscious and foreconscious." The unconscious is not merely physiological—far from that! It is a kind of unconscious consciousness, possessing the characteristics of consciousness when you need them for the purposes of your argument, but not when they would disturb the argument.

The Freudian confusion regarding the unconscious is in part chargeable against the long-standing ambiguity of the term, "consciousness," which has meant sometimes "awareness," and sometimes "objects of which one is directly aware."

> From this grand ambiguity trouble has arisen continually. The strife between " interactionism " and " parallelism ": the conception of thought and consciousness as stuff and things, and conversely, the conception of perceived objects as figments of " mind ": are only details of the results, of this confusion. . . . We eliminate at the start the metaphysical theories of epistemological dualism which have played so pernicious a role in the history of the Anglo-German psychology. Experience does not give us directly two worlds of objects physical and mental . . . but merely a world *of objects of which we are aware* (pp. 117, 120, 121).

Though the author does not speak of this conclusion of his as having any history, it seems rather reminiscent of things that have been said by Wundt, James, and other representatives of the objectionable "Anglo-German psychology."

> The ambiguity of the term " consciousness " is undoubtedly a factor con-

> tributing to the Freudian confusion over the " unconscious." . . . But this verbal confusion although a great help to the theory of " unconscious mind " is not its vital source. . . . The believer in " unconscious mental processes " . . . might state his claim as follows: In addition to mental processes, *i.e.*, organic processes involving consciousness (awareness), there are other processes, which while they do not involve awareness, involve something which is more than mere physiological process: something resembling consciousness, but not conscious. This " unconscious mental " factor is therefore an x, an unknown, and can not be pointed out in any definite experience. Such an hypothesis might be made. One might also hypothesize a y factor, a z factor, and an infinity of other factors, all equally unknown, equally beyond experience " (pp. 125, 126).

Since, then, the hypothesis of an unconscious which is not purely physiological is "removed from any possibility of verification," it is entirely valueless to science. What its value or appeal may be to those who eagerly embrace it the author does not stop to inquire, but the answer is suggested in a highly critical review of this same book by the psychiatrist, John T. MacCurdy, M.D.[1] Dr. MacCurdy bases an argument for the unconscious, as against "physiology," on a law of creative synthesis, which can be seen in operation throughout the realm of natural science. Any synthetic combination shows properties that could not be predicted from the known properties of the synthesized elements—as, for example, the crystalline properties of common salt could not be inferred from the known properties of sodium and chlorine. Analytical chemistry tells you of the elements composing the compound, but, to know the compound itself, you must study it for itself, and not in terms of its elements. In the same way, biological processes may be analyzed into physical and chemical processes, but if for that reason we neglected to study life processes as such, we should never know much biology. In just the same way, mental processes are composed of physiological processes; but if we are satisfied to study them only from the physiological side, we shall never know much psychology. Psychology, the contention is, must stick to its own order of facts, and to its own concepts, and not take refuge in physiological interpretations. Therefore, it has need for the concept of unconscious mental processes. The value of the "unconscious," accordingly, is simply that it enables one to keep on using psychological terms, even when dealing with unconscious processes.

I shall not undertake any criticism of this interesting contention, except to remark that it serves better as a justification for the "co-conscious," in Morton Prince's sense, than for Freud's particular brand of unconscious. In double personality there is some evidence of a secondary synthesis of processes lying outside the main consciousness, and thus some reason for speaking of co-conscious mental processes; but in the case of dreams, lapses, queer impulses and fears, and unanalyzed motives, I do not know that anyone has seriously attempted to demonstrate the existence of any synthesis additional to that which occurs in the conscious process itself. The conscious process may show disturbance from some source outside itself; but, unless it could be shown that this outside source consisted of processes integrated into a total process of the mental order, we should, even according to Dr. MacCurdy's principle, regard it as physiological.

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[1] In an article entitled, " Psychiatry and Scientific Psychology," in *Mental Hygiene* for April, 1921, Vol. 5, pp. 239–265.

As against the unconscious in the sense of a storehouse of memories, Professor Dunlap puts the case with striking clearness:

> An idea is not a thing like a written document which, after being in the active files is taken out and stored in the transfer case. It is more like an *act* such as snapping the fingers or striking a blow. I may snap my fingers ten times in succession: but no one supposes that the snaps have an individual existence afterwards and are somewhere stored away as snaps which are no longer snapping. No more does scientific psychology conceive of " ideas " as something which can be stored away after they are through " ideating." In the one case as in the other, there is a physiological basis which is modified by the act in such a way that the act can be repeated at a future time (pp. 105–106).

Professor Dunlap, as the reviewer happens to know, has had his eye on the psychoanalysts for many years, and his remarks regarding them are by no means purely "academic." Some of these remarks deserve quotation.

> Reactions which later become a part of the general sex activity are found in the child, and therefore pointed out as evidence of sex activity. It is as if one should claim that the labored breathing produced by running to catch a street car is *sexual* because the same labored breathing may occur during certain stages of sex activity (p. 60).
>
> As is readily seen, anything that can be dreamed of has a ready sex interpretation. So that the telling of one's dreams to anyone versed in the gentle art of psychoanalysis is a matter in which your feelings of delicacy or prudence will dictate if you realize the possibilities (p. 71).
>
> This . . . characteristic is true in my opinion of all the cases in which the Freudian analysis " strikes oil." The situation which is discovered through analysis is one which is perfectly well known to the patient, but the patient is loath to confess it and does not realize its importance (pp. 79–80).
>
> The psychoanalyst like the philosophical mystic is essentially tender-minded, and can not endure the difficulties and disappointments of prosaic science. We are not surprised, therefore, to find over and above the essential logical fallacy on which the system is based, a characteristic naïveté in reasoning and a characteristic lack of orientation in facts (p. 93).
>
> It is probable that psychoanalysts do produce cures, or at least marked alleviation of the condition, of certain cases. In other cases the results are less desirable. The question of vital importance is whether the harm done by the general application of the method outweighs the good accomplished (p. 102).
>
> Sometimes, a complex is built up by prolonged psychoanalysis. The patient,

> for example, is convinced that his neurosis is a result of the mother-complex; at first he is astonished at the psychoanalyst's discovery but by the copious use of symbolism, by the perversion of all the patient says and does, with that end in view, he is finally persuaded that the complex originated in him, and not in the psychoanalyst. By constant contemplation of the complex and its magic relationships, all the symptoms of the patient's troubles become closely associated with it. If now the psychoanalyst can exorcise the demon he has raised the patient may be cured. . . . In many cases, however, the demon refuses to be exorcised or if he complacently leaves, returns shortly with "seven worse than himself," and the latter state of the patient is worse than the first (pp. 102–103).
>
> Psychology has been culpably negligent in regard to the study of the desires, and the one positive service which the Freudians have done is in emphasizing the incompetence of our information (and also of their own information) on this important subject (p. 159).

The psychologist who needs a little excitement should not fail to read Dunlap's book, and follow it with some of the reviews of it by psychoanalysts. Even MacCurdy, whose long review contains much serious criticism, starts off by attempting to obscure the issue with a dust-cloud of professional jealousies. He speaks of an "antagonism at present existing between psychiatrists and what we may term 'academic psychologists' . . . ." "Both the psychiatrists and the psychologists insist that they are the ones who should direct the study and treatment of those mental abnormalities which lead to social unrest, economic insufficiency, and crime, as well as to frank nervous and mental disease." And this in face of Dunlap's statement: "The development of a sound psychotherapeutics will certainly not be the work of the general psychologist. But when it is developed, it will be developed by psychopathologists of thorough training in general psychology" (p. 165). Later on in his review, referring to Dunlap's strictures on the practise of psychoanalysis, MacCurdy exclaims: "How can a college professor, presumably attending to his teaching and laboratory work, who has never had a medical education and has never seen a patient except with a layman's eyes—how can he have collected material justifying such sweeping conclusions?"

Observe, however, the reaction of an anonymous psychoanalyst in the *Journal of Nervous and Mental Diseases:*

> The work in question is just such as one would expect of a blind rat caught in a maze—defeat, failure, going here and going there, misunderstanding, misquoting, misreading, a tissue of stupid distortions, clumsy misrepresentations, and silly evasions based upon a host of phobic prejudices. . . . The book, to us, is a ludicrous defense reaction. Psychoanalysis he particularly singles out as a special obstacle to science. Piffle! Nothing can obstruct science when it seeks to learn about reality, not even Dunlap's hobgoblin antics. The psychoanalytic hypotheses are rather object lessons to make-believe scientists who entrenched behind their academic chairs are screeching aloud their condemnation of many things they not only do not understand, but of the very essence of which they seem fundamentally

> incapable ever of understanding by reason of their emotional limitations. . . . One would like further to characterize this unutterably stupid production, but already too much space has been wasted in noticing it at all.

Certainly Dunlap has produced a very stimulating essay!

His second main discussion, on the "Reaction Arc Hypothesis," is more apt to arouse the psychologist. The reaction arc concept is the old reflex arc concept, "transformed by scientific psychology" some time within the decade, 1910–1920. Apparently the transformation in question has been the author's own achievement, and has consisted in bringing perception and the association of ideas into line under the general conception of *complete sensorimotor reactions.*

The reaction arc hypothesis is opposed to the old "phrenological" conception of consciousness as "dependent on the specialized functions of certain groups of neurons set apart from the other sorts of neurons" (p. 132). The function of any one neuron is the same as that of any other neuron, *viz.,* "to be irritated or stimulated, and to irritate in turn another cell" (p. 130). "Mechanically, the function of the nervous system is the production of *responses*: that is, the action of effectors in certain ways, consequent upon specific action of receptors. . . . Certain definite responses, or *reactions* of the organism, are accompanied by, or involve, consciousness. . . . From these facts, the construction of the reaction-arc hypothesis is inevitable. Consciousness (awareness) is the result of, or the accompaniment of, or a part of (the phrasing is for the present immaterial)[2] certain reactions involving the activity of a complete arc from receptors to effectors" (pp. 133–134).

Seeing an object, for example, is a sensorimotor reaction, and requires the activity of a "complete arc."

> In the case of vision, experiments on animals, and human clinical cases, make this point clear. Destroying the retinæ; cutting the optic nerves; cutting the optic tract behind the brain-stem; destroying the occipital lobes (of the cerebral hemispheres) to which the optic tracts lead; or cutting the connections between the occipital lobes and the rest of the cerebral hemispheres: produce the one and the same results—blindness—by interrupting the arcs from the visual receptors to the effector systems, and destroying the possibility of a visual reaction. There is no single system of efferent channels from the hemispheres which the visual reaction need follow: hence, to block completely the visual reactions by operation on the efferent side of the arc, all the efferent channels from the hemispheres would have to be cut. This would cut off the possibility of not only visual, but all reactions—and the patient would not survive (p. 135).

This last is certainly unfortunate, as it means that the "reaction arc hypothesis" can never be verified. We must, it would seem, relegate this hypothesis to the limbo of hypotheses "which are by their nature removed from any possibility of verification," and which are consequently valueless. We shall never know whether a person could see, with all the motor channels from his brain blocked off; and, since we can not know, why should we care?

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[2] The reviewer would prefer to say, " an attribute of."

Still, though the reaction arc hypothesis can never be positively verified, it might perhaps be *disproved* by some less drastic experiment. Some deduction from the hypothesis might conflict with facts. The author helps us on our way, for he deduces various consequences of his general hypothesis, and insists, moreover, that in thus applying it to various probloms he does not need to introduce any accessory hypotheses.

He applies the hypothesis to the case of the conditioned reflex. Here we have two stimuli, originally giving two different reactions, but, through simultaneous activity, the two arcs "become connected in the cerebrum so that the current flowing in over the afferent part of the one, may now flow out over the efferent part of the other" (p. 143). Such interconnection of reaction arcs in the cerebrum explains "all habit formation, including both the development of perception, and the association of ideas" (pp. 143–144).

In the development of perception, the child may see and smell an orange simultaneously, and the arcs for the visual reaction and for the olfactory reaction, becoming connected in the cerebrum, enable the child later to make the olfactory response to the visual stimulus.

When he comes to the association of "ideas," the author makes use of the well-known scheme for the chaining together of a series of movements into a skilled performance, such as dancing. The reaction *A*, terminating in the contraction of certain muscles, excites the muscle spindles in those muscles, and thus initiates a second, proprioceptive reaction. Along with this proprioceptive reaction, there is simultaneously excited by an external stimulus the movement *B*; and the afferent part of the proprioceptive arc becomes connected in the cerebrum with the efferent part of the reaction *B*, quite after the pattern of the conditioned reflex. In this way, through the mediation of the proprioceptive arc, movement *B* becomes attached to movement *A*, and no longer requires its own original stimulus.

Now, since an "idea" is to be conceived as a sensorimotor reaction, it must give rise to a proprioceptive reaction in the way just explained, and this proprioceptive reaction (or the afferent half of it, extending from the muscle spindles to the brain), must be the intermediary between one idea and the next in an associated chain.

> If ideas are dependent on reactions, and if ideas are capable of association, it must be that the ideational reaction-arcs are of such a nature that the completion of one reaction may initiate another. Since reaction arcs terminate in muscles and in glands, it must be that in one of these tissues lie the necessary receptors of the thought-arcs. The receptors in glands are as yet conjectural, and the glandular response is not of such a nature that we could assume it to be the stimulus of reactions as prompt, as manifold, and as finely graded as thought-reactions apparently are. The striped muscles, however, are provided with a plentitude of receptors in the "muscle spindles," and the muscular responses are quick, finely graded, and of great complexity, competent to initiate reactions of an endless variety. The muscle-receptors are, therefore, in all probability, the beginnings of the thought-arcs (p. 147).

Here we have a very specific deduction from the general reaction arc hypothesis, and it appears that this deduction can be put to the test of facts. Since the muscle-spindle intermediary between two ideas is necessarily "finely graded, and of great complexity," and also highly specific, in order to give the specific sequence of "ideas," it certainly should follow that the amputation of an arm or a leg would break up many definite associations. Loss of the muscle sense, as in tabes, should break up associations of ideas, as it is known to interfere with skilled sequences of movement. Dr. MacCurdy, in the review already mentioned, brings forward still more striking evidence:

> If thought originates in muscle perceptions—and he evidently means this literally—then destruction of the muscles must abolish ideas. There are many nervous and muscular diseases in which muscle tissue disappears almost entirely. In these thinking is usually undisturbed. It is possible for an individual to live with his spinal cord divided in the neck so that there is no connection between the brain and any voluntary muscle except those of the head and neck and the diaphragm. Thinking is still possible and may be acute. In case it were urged that thoughts were initiated by the vocal, mouth and neck muscles, it is easy to point to diseases where these are atrophied or have their nerve supply cut off from the brain (p. 255).

This appears like a complete disproof of the reaction arc hypothesis as applied to the association of ideas. The only line of defense which our author has prepared against such an attack is briefly indicated in the following passage:

> The reactions are ultimately abbreviated or short-circuited. . . . The complete muscular reaction is necessary during the learning process, but is largely eliminated, in the interests of economy, after the series have been thoroughly mechanized (p. 153).

But against the adequacy of this defense it may be urged: (1) that, according to the hypothesis, at least some imperfectly mechanized associations must be lost by the amputation of a limb; (2) that amputation, *etc.*, should be a very serious handicap in the formation of new associations; and (3) that in admitting that, after mechanization, a thought can function otherwise than as a complete arc, the author has notably receded from the boldness of his first position. At first he held that "ideas are dependent on reactions," *i.e.*, on complete sensorimotor reactions—since, according to the reaction arc hypothesis, all mental processes are so dependent. But now we learn that the complete reaction is largely eliminated, and still the ideational processes occur. Apparently, some deductions from his general hypothesis have seemed unacceptable to the author, and he has therefore introduced a qualification which amounts to abandoning the hypothesis in its original clean-cut form.

As a matter of fact, he did not succeed in holding fast to the hypothesis even while applying it to the conditioned reflex and to perception. He dealt not in complete arcs as his units, but in half-arcs. The afferent half of one arc became connected in the cerebrum with the efferent half of another arc. In the same way, though he began his interpretation of the association of ideas with the presupposition that he must deal in complete arcs, he had immediately to split up his proprioceptive arc, and let the afferent half of it function alone in mediating between one idea and another. Any theory which dealt consistently with complete arcs would have to seek the formation of new linkages *in the periphery!*

It is impossible to write a physiological psychology in terms of the "complete arc" as unit. Learning, whether motor, perceptive, or "ideational," requires a smaller unit. Facilitation and inhibition require a smaller unit. In the simplest cases of learning, such as the conditioned reflex, the half arc seems to make a workable unit. In reactions that involve a series of cerebral steps between the sensory stimulus and the motor response (as in the case of seeing a signal, knowing what it signifies, and responding with an appropriate skilled movement), the unit is smaller than a half arc, and amounts to a single one of the cerebral steps. In our author's fundamental statement, "that the function of any one neuron is the same as that of any other neuron, *viz.*, to be irritated or stimulated, and to irritate in turn another cell," there is certainly nothing that demands the "complete arc" as the unit. Rather, the unit should be the stimulation of one neuron by another. This is the single step in any psychoneural process; and there is no reason compelling us to adopt any larger unit as the ultimate unit of physiological psychology.

R. S. WOODWORTH.

COLUMBIA UNIVERSITY.